by the agency of small animals quoted as if they were well-established facts (p. 184). Chara is spoken of as "a sort of transition stage" between the red and brown seaweeds (p. 199). We fail to find any grounds for this extraordinary statement. We are informed that in Selaginella "the rain or dew will settle in the hollow of the leaf, and help to float the zoosperms; but in Pinus their dry and motionless representatives are more exposed to the wind on the outer surface of the leaf" (p. 279). It would be difficult to frame a sentence more hopelessly inaccurate than this.

The following passage from the introductory chapter is worth quoting: "Morphology by itself is thus seen to be a matter of mechanism, revealing nothing higher than a combination of mechanical movements, harmonious in action and beautiful in execution; but physiology, dependent on structure for the interpretation of the phenomena of life and the causes thereof, seeks to reveal the inner life as well as the outward expression of it" (p. 10). It will probably be new to most of us to learn that morphology reveals movements at all, mechanical or otherwise.

At p. 179 "apogamy" and "self-fertilisation" are used as equivalent terms; at p. 138 Spirogyra is said to produce gonidia, and at p. 25 reproduction in Selaginella is said to take place by means of a true seed.

We have only cited a few examples of positive errors, but throughout the book the terminology is strange and confusing, even where not absolutely incorrect.

The book is presumably intended for students "cramming" for examinations, but even for this purpose we fear that it will prove worse than useless. D. H. S.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.*]

Vitality, and its Definition

IT is perhaps desirable that I should offer a few words of explanation, by way of reply to several of your correspondents, who have commented upon certain statements in my recent address to the Geological Society.

In the first place, I think that any candid reader of that address will acquit me of being guilty of such presumption as to make a statement, on my own authority, concerning the vitality of seeds. My object was to contrast the greater stability of mineral structures with the lesser stability of animal and vegetable structures. Consequently I selected what I thought would be regarded as the *extreme* examples of prolonged vitality in the animal and vegetable worlds respectively. It was quite sufficient for my purpose that competent botanists have cited the case of the germination of seeds taken from ancient Egyptian tombs as authentic, and that a botanist of such eminence as A. de Candolle should assure us that it is "not impossible." As a matter of fact, I have been informed, however, by a reliable authority that experiments on the germination of seeds taken from mummies have very recently been conducted to a successful issue.

With respect to Mr. Herbert Spencer's definition of life, my object was not to find fault with it but to show that the differences between "organic" and "inorganic" matter are of so shadowy a kind as to defy definition. Even straining the meaning of the word "correspondence" so as to give it the force implied in the passage cited from the "Principles of Biology" by your correspondent Mr. Collins, I maintain that in those changes undergone by minerals to which I apply the term "physiological" there is a complete "correspondence with external sequences." When the temperature of a crystal is altered through a certain range, expansion and contraction take place unequally in accordance with the molecular structure of the mass. In consequence of this unequal expansion and contraction, stresses are produced and the crystal undergoes an internal molecular rearrangement, which is determined by a latent "organisation," though it can only be detected, perhaps, by its action on the light-waves. But now let another set of forces come into play, namely, the chemical action of liquids containing gases in solution, and immediately the effects of the former change are seen in the manner in which the crystal yields to the new forces operating upon it. This *secondary* change is in fact only rendered possible by the primary one having taken place. But the changes produced by solvent action in turn weaken the stability of the whole mass, permitting other chemical affinities to assert themselves, in consequence of which the crystal enters upon a long series of metamorphoses which terminate in the complete "dissolution" of the ties that held together its molecules; it thus becomes a pseudomorph, a sort of mineral corpse, with the external form of the original crystal only, but without any of that capacity for undergoing a wonderful cycle of changes which was its original endowment. After this the materials of the "dead" crystal may be used up to form the substance of new ones.

It is scarcely necessary to add that I had no serious intention of asserting that minerals do actually live, in the sense in which "living" is popularly understood. All I care to insist upon is that minerals, like animals and plants, go through definite cycles of change, dependent on their environment, and that the distinction between "organic" or "living" matter and "inorganic" or "lifeless" matter is therefore not a fundamental one. Surely no better proof of this can be adduced than the fact that the more exact we try to make our definitions of the terms "life" and "organisation," the more shadowy and intangible become the distinctions upon which we are driven to depend. I am perfectly satisfied with Mr. Herbert Spencer's admission of "insensible modifications and gradual transitions which render definition impossible." But if this be the case, it is surely not wise to maintain that the science of "non-living" beings must differ totally in its aims and its methods from that of "living" beings. To bring out into clear relief the analogies between the science dealing with the mineral kingdom and those concerned with the animal and vegetable kingdoms was the main object of my address. JOHN W. JUDD

March 28

"The *Gecko* moves its Upper Jaw"

THUS by the substitution of one reptile for another—of the gecko for the crocodile—the well-remembered zoological statement in Arnold's Greek prose is at length put upon a satisfactory foundation. In the spring of 1886, I captured a small gecko (*Tarantola mauritanica*) at Rome, and I have hitherto succeeded in keeping it alive and in health. One of the first things I noticed about it was the extraordinary vigour with which so small an animal would bite one's finger. And the effect produced was certainly rather due to the lizard's expression of intense ferocity during the process than to the pinch which it was able to give. The expression chiefly depends upon two things—the fact that the anterior part of the head may be bent downwards, and that the eyes are retracted into the head. Examining the former movement more carefully, it was seen that in opening the mouth the upper jaw is distinctly although slightly raised above the normal, so that the profile of the upper surface of the head becomes almost straight (compare Figs. 1 and 2). In biting fiercely it is common for the upper jaw to be depressed below the normal, as is plainly seen in a profile view (compare Fig. 3), although in other positions the curvature of the head is

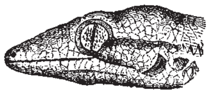

FIG. 1.—The Tarantola in the normal position of rest.

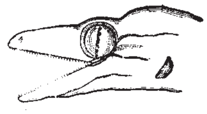

FIG. 2.—The Tarantola prepared to bite, with the upper jaw raised.

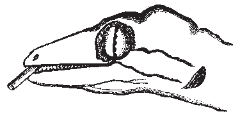

FIG. 3.—The Tarantola biting,—a common position, in which the upper jaw is depressed below the normal.

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normal, and again in others the profile may remain straight in biting (as in Fig. 2). As far as I could observe in Tarantola, the upper jaw was always raised in opening the mouth, and the profile of the head straightened from its normal curve when at rest, but on closing the mouth in biting the movement of the upper jaw depended upon the relative position of the animal to the object which it was biting. This depression of the upper jaw may be also often witnessed when the mouth is closed, and it may be produced by applying slight pressure to the head. The animal seems to make the most of its powers of expression, for on provocation it opens its relatively huge mouth with the greatest readiness, and will keep it open for a considerable time, during which its appearance is sufficiently awe-inspiring. The fact that the anterior part of the skull is not co-ossified with the posterior part is well known. Thus in Huxley's "Anatomy of Vertebrata" (1871, p. 225) the following statement is made concerning the geckos: "Neither the upper nor the lower temporal arcades are ossified, the post-frontal being connected with the squamosal and the maxilla with the quadrate by ligament;" and Mr. Boulenger informs me that had he been asked whether the upper jaw of such lizards is moved in biting, he would have been inclined to answer in the affirmative, reasoning from the well-known condition of the skull. But I believe it has not been hitherto actually observed that such movable articulations possess a functional value in the living animal, and that the geckos must be added to the well-known instance of the parrots as Vertebrata which move the upper jaw in biting. It is extremely probable that the same observations will be found to hold for other families of lizards. EDWARD B. POULTON

Wykeham House, Oxford, March 1

Weight and Mass

TILL some quite new facts are discovered, such as shall force us to reconsider our convictions (which have not been lightly formed), I do not think it profitable to accept a quasi-metaphysical challenge from my friend Prof. Greenhill. He has at heart, as strongly as I have, the cause of *definiteness* and *accuracy*:—and if he, as is natural for one in his position, feels inclined to sympathise with the "vernacular" of Engineers rather than object to it as I do, there is nothing for it but to agree to differ. My remarks on this aspect of the subject were of the most cursory and general character; and I went so far as to say that, as the book in question was written by a practical man for practical men, "perhaps we ought not to complain."

I cannot, however, go *so much further* as to allow, with Prof. Greenhill, that it is "perfectly correct" to use the words pound or ton "side by side in two senses." As regards this practice I, in turn, must quote from an unpublished letter of Clerk-Maxwell's. [The passage purports to be part of a (reported) speech by a well-known Evolutionist.]

"He regretted that so many . . . . were in the habit of employing the word in a sense *too definite and limited* to be of any use in a complete theory. . . . . He had himself always been careful to preserve that largeness of meaning which was too often lost sight of in elementary works. This was best done *by using the word sometimes in one sense and sometimes in another;* and in this way he trusted he had made the word occupy a sufficiently large field of thought."

I have three other remarks to make upon Prof. Greenhill's letter:—

(1) He shows the absurdity of defining the weight of a body as "the force with which it is attracted by the earth." Of course such a definition must necessarily be absurd provided it comes *after* an explanation (given by Prof. Greenhill) that "weight" is to be understood in the sense of "mass." But from this explanation itself it would unfortunately follow that *a body has weight even when it is no longer heavy;* as, for instance, when it is in a (supposed) cavity at the centre of the earth! Prof. Greenhill says that "weight" is "used in ordinary language in most cases" in the same sense as "mass." Surely the great majority of men regard weight from the point of view of the sublime Porthos:—

"Ma vaisselle d'argent . . . . qui doit peser de mille à douze cents livres, CAR je pouvais à grande peine soulever le coffre qui la renferme, et ne faisais que six fois le tour de ma chambre en le portant."

(2) He also speaks of certain difficulties imposed by the "rules of language." I do not ascribe to them so lofty an origin. They are the offspring of the dogmatic ignorance which has peopled the realms of science with Centrifugal Force and its fellow monsters.

(3) He has commented solely on a passing remark in my article, and says nothing as to its main purpose. I hope, however, that he will eschew "statical" measures of force, and give his hearty aid as well as his good wishes in the war of extermination which must perpetually be waged against the too luxuriant undergrowth of the scientific garden:—the circle-squarers, the perpetual-motionists, and (in the case before us) *the measurers of potential energy in terms of horse-power.*

P. G. T.

An Error in Maxwell's "Electricity and Magnetism"

IT may be allowed to me to remark that the error mentioned on pp. 172 and 223 of NATURE has its origin really in Helmholtz's renowned paper ("Ueber die Erhaltung der Kraft," 1847, p. 67), and that it thence found its way into most of the text-books on electricity. It has sometimes been detected and hinted at; for the first time, I believe, in C. Neumann's paper published in the *Ber. d. k. sächs. Ges. d. Wiss*, at Leipzig (1871), "Elektrodynamische Untersuchungen mit besonderer Rücksicht auf das Princip der Energie." There we find (p. 436) the formula—

$$T\nabla + T_1\nabla_1 = T^2w + T_1{}^2w_1 - TT_1\frac{dV_{01}}{dt} + \frac{dF}{dt},$$

which is identical with the equation of energy given in NATURE, p. 223, if we put—

$$\nabla_1 = A_1,\ w_1 = R_1,\ -\ TT_1V_{01} = T_m,\ F = T_e, \text{ \&c.}$$

The formula is followed by the remark that it agrees entirely with the formula given by Helmholtz, the only difference being the last term $\frac{dF}{dt}$, which in the latter is wanting; thus we may say that by Helmholtz the potential energy of the system invariably is expressed by zero.

Being formerly unaware of Neumann's researches, I, by another way, came to the same results, see the paper "Das Princip der Energie in seiner Anwendung auf die ponderomotorischen und elektromotorischen Wirkungen des elektrischen Stromes," published in the *Sitzber. d. k. böhm. Ges. d. Wiss*, (*vide* NATURE, vol. xxxii. p. 308). In this paper I have hinted at *one* probable cause of this and similar mistakes and their relatively difficult discovery, of which the repetition of the error in the *best text-books* is a decisive proof. This cause I believe to be the trivial circumstance that there is no consistent and generally accepted notation of the different forms of potential and energy. This renders the comparison of different writings on this subject sometimes quite perplexing. When, for example, two authors denote the *same thing*, the one by V, the other by $-V$, and when the first writes the expression $d(T_1T_2V)$ in the form—

$$T_1T_2dV + T_2VdT_1 + T_1VdT_2,$$

the other the *identical* expression $-d(T_1T_2V)$ in the form—

$$T_1T_2dV - T_2d(T_1V) - T_1d(T_2V),$$

it may happen that they themselves and also other readers overlook the difference of sign, and that they continue to reason as *if their* V *were identical.* Such mistakes occur oftener than is supposed. See the interesting note at the end of Sir William Thomson's paper on "Capillary Attraction," NATURE, vol. xxxiv. p. 369.

Could there not be found a means of avoiding the inconveniences caused by such mistakes and the loss of labour spent in detecting them? I dare not hint at the possibility of an international system of notations of the most important physical quantities; thoroughly consistent, and recommended by the highest scientific authorities; for the realisation of such a system would probably meet difficulties quite insurmountable.

Prague University, March 12 A. SEYDLER

Tabasheer

MR. DYER in his article on "Tabasheer" in NATURE of February 24 (p. 396), throws out the suggestion that the silica deposited in the joints of bamboo may have undergone a process of dialysis. It may be of some interest to him, and to your readers generally, to learn that plates of transparent compact silica, SiO_2, may be formed by dialysing the basic soda silicate. Four or

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